

MONTHLY NOTICES

OF THE

ROYAL ASTRONOMICAL SOCIETY.

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No. 3.

WARREN DE LA RUE, Esq., President, in the Chair.

Thomas G. Rylands, Esq., Heath House, Warrington;

Rev. A. W. Deey, Alton, Hants;

Frederick Robert Hughes, Esq., Borrowstownness, N.B.;

The Hon. John William Strutt, Tirling Place, Witham,
Essex; and

Capt. Charles Thomas Curme, R.N., Milborne Port, Somerset,

were balloted for and duly elected Fellows of the Society.

Proposition for a Telescope on the Andes. By Lieut. E. D. Ashe,
Director of the Quebec Observatory.

(Letter to the President.)

As President of the Astronomical Society, I venture to suggest to you that a first-class telescope should be placed on one of the higher Llanos of the Andes, some of which are admirably adapted for astronomical observations; and as the Geographical Society has had no difficulty in raising funds for several of their praiseworthy expeditions, neither do I anticipate any difficulty in defraying the expenses of an astronomical party, whose object would be to observe the discs of planets and the surface of the Sun.

Much was done by the Teneriffe expedition; still incomparably greater results might be expected from placing a glass at a much higher elevation, and removed from the effects of a moist atmosphere.

It was my good fortune, when belonging to a frigate on the South American Station, to have crossed the Andes, in Peru, in about Lat. 18° South.

The Pass of Tarcora has an elevation of about 20,000 feet, and is reached easily in three days from the delightful city of Tacna, situated on the coast.

From the Pass to the Lake of Titicaca are numerous plains or Llanos, at different heights, varying from 12,000 to 20,000 feet; one of which might be selected for the purpose. I should be very happy to give any information or render any assistance.

As a rough sketch of the route of the expedition to its destination, I may say that the party should go by the West Indian Mail to Panama; there a man-of-war steamer would be ready to take them to Arica, on the coast of Peru, and from there the expedition would proceed to the pretty town of Tacna, distant forty miles, where it could remain and enjoy itself, and send out surveying parties, who would bring back information, upon which it would select a spot, and proceed to its destination.

There would be no difficulty in conveying the different parts of an Equatoreal, as a mule can carry 4 cwt.; and as I have seen a "Collard and Collard" pianoforte upon a mule's back crossing the Andes, there can be no doubt that the different parts of the telescope could be carefully carried.

No one need join the party who could not stand a little roughing, and endure the fatigue of thirteen hours upon the back of a mule for several days together.

I enclose an account of my journey across the Andes, which may give some information to those desiring to join the expedition; and, in conclusion, I may say that it is probable that the Canadian Government would allow my telescope of 8 inches aperture and my services upon so laudable an undertaking.

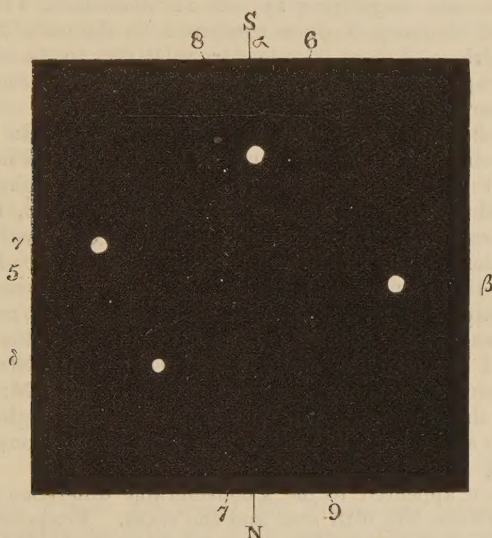
London, November 10, 1865.

The President: I have since received a letter from Commander Ashe, and he very strongly urges the desirability of an expedition to Peru. He has a very high opinion of the atmosphere there, and thinks that we should obtain observations such as we could get in no other part of the world. The letter is of value, from its containing the experience of a man accustomed to make astronomical observations.

Mr. Evan Hopkins, who has travelled in Peru, bore testimony to the excellence of the climate.

On the Stars within the Trapezium of the Nebula of Orion.
By William Huggins, Esq., F.R.S.

Since a prismatic analysis of the light of the great Nebula in *Orion* has shown that this remarkable object has a gaseous constitution, the changes in the arrangement of the luminous matter and in the relative brightness of different parts, which some observers are of opinion are taking place in this nebula, possess great interest and importance. For it will be probably from a more careful and systematic observation of the alterations in form and brightness which may take place in this



The Trapezium of Orion, as seen by Mr. Huggins,
Jan. 6 and 8, 1866.

object, and in the other gaseous nebulae, that the necessary evidence will be obtained for a future determination of the true cosmical rank and relations of these bodies.

The discrepancy which exists between the published observations of the minute stars within the trapezium and in its neighbourhood, appears to show that the stars are subject to considerable variation in brightness.

M. Otto Struve remarks, "We have amongst the six observed stars in the Regio Huygheniana, on a space of three to four square minutes, at least four variable stars; but I think the number of variable stars in the same region will yet be considerably increased by further observations. The sixth star of the trapezium bears strong indications of variability. . . . Probably two or three stars, seen and measured by M. Lamont in 1837, will be found of variable light. . . . Perhaps some of the stars indicated by De Vico, Bond, and others, in the immediate vicinity of the Trapezium, might be found of variable light."*

On the evening of January 6, 1866, the atmosphere was unusually favourable for the detection of minute points of light, and I was able to estimate satisfactorily the positions and relative brightness of three very small stars situated within the space enclosed by the well-known six stars of the Trapezium. They were detected with a Kellner eye-piece magnifying 60 diameters, and were afterwards seen with double-convex lenses, magnifying 135 and 220 diameters. The observations of January 6 were confirmed on January 7 and 8.† The positions of these minute stars relatively to the other stars of the trapezium are shown in the diagram which accompanies this paper.

The fifth and sixth stars were steadily seen. In 1852 Mr. Lassell obtained some fine views of these stars, and he puts on record that "the sixth star seemed equally bright with the fifth, and quite as easily seen."‡ On December 8, 1856, Mr. Lassell remarks, "The sixth star appears decidedly the brightest, or the largest, intrinsically, and if in the place of the fifth would I think be more easily seen. If these stars are unaltered in magnitude I cannot understand why I should, many years ago, have so often scrutinized the fifth star with this telescope before I detected the sixth."§ In 1857 M. O. Struve says, "On former occasions I agreed perfectly with Mr. Lassell's remark that the sixth star was fully of equal brightness with the fifth star; but this year it appeared to me constantly inferior."||

There appeared to me a considerable difference in brightness between the fifth star and the sixth. Frequently during the passage of masses of haze across the stars the fifth was seen steadily when the sixth could not be detected. My observations agree with the magnitudes assigned to these stars by Sir John Herschel in 1834. Sir John Herschel says, "Applying the aperture 12 inches, the sixth star was finely seen. It is ex-

* *Monthly Notices*, vol. xvii. p. 227.

† On this evening my friend, Mr. S. B. Kincaid, visited the Observatory, and saw all the stars figured in the diagram.

‡ *Monthly Notices*, vol. xiv. p. 74.

§ *Ibid.* vol. xvii. p. 68.

|| *Ibid.* vol. xvii. p. 227.

cessively minute and very close to α , much more so than the fifth is to γ , say half the distance, and 14th mag., while the fifth = 12th mag."*

The minute star nearly in the middle of the Trapezium, and marked 7 in the diagram, appeared to me much fainter than the sixth star, probably nearly as much so as the sixth is now smaller than the fifth. I do not know whether a star has been seen before in this position within the Trapezium.

The star numbered 8 in the diagram is probably in a small degree brighter than 7, but the nearness of 8 to the bright star α makes the steady vision of it very difficult. This star can scarcely be the same as the one which M. Porro saw in 1857. In M. D'Abbadie's plan of the Trapezium, M. Porro's star is placed nearly halfway from α to γ .†

The star 9 of the diagram is rather feebler than 7. I was able to see it only by occasional glimpses. I should think there can be little doubt that this is one of the stars observed by De Vico in 1839.‡ I had recorded its place before I saw De Vico's diagram. The position which I have given to it is not identical with that of De Vico's star, but the extreme difficulty of this object made the determination of its position a little uncertain.

No one of the minute stars which I observed agrees in position with the "new star" discovered by Mr. Lassell at Malta on January 29, 1862. A diagram of this star is given in the *Monthly Notices*.§

We do not know whether these minute stars are physically connected with the great nebula, or are only in optical association with it. As a means of obtaining information on this point, it appears to be of great importance to determine whether the variability of the stars in the nebula and the occasional appearance of new stars can be associated with any changes which may be observed in the nebula.

Captain Noble: My friend Mr. Brodie has informed me that by gauging the fifth and sixth stars by Dawes's method of diaphragms he has found that the sixth star was, on the night he tried it, of precisely half a magnitude less than the fifth. I have seen the sixth star with my $4\frac{2}{10}$ -inch aperture, and therefore, from what Mr. Huggins says about variability, I think the thing is put out of the range of hypothesis, and is almost

* *Cape Observations*, p. 30.

† *Monthly Notices*, vol. xvii. p. 245.

‡ "Memoria intorno a parecchie Osservazioni fatte in Collegio Romano, anno 1839."

§ Vol. xxii. p. 164.

a certainty. I have tried many times to see that star, and could not, and yet I have seen it at others:—the fifth I can always see.

The President: I quite agree that the whole system of the trapezium with the included faint stars is variable. At times I have seen the sixth star very much brighter than the fifth, notwithstanding its proximity to its companion star, and I have also caught sight of the fainter stars, stars within the trapezium, without noting down their positions at the time. I believe those stars are variable because on other occasions I have searched for them in vain, and I think this will be found to be the case. Whether one can establish a physical connection between these various stars it is difficult to say, for some of them are so extremely difficult to be seen, and their variability renders it still more difficult to say whether it is the same star which is seen on different occasions. Diligent research and observation with respect to the nebula of *Orion* will certainly bring to light some interesting phenomena. I would also recommend the close observation of other groups of faint stars; for in many of these—I did observe them years ago more than I do now—I have noticed traces of variability.

Note regarding the Decrease of Actinic Effect near the Circumference of the Sun, as shown by the Kew Pictures. By Messrs. Warren De La Rue, Stewart, and Loewy.

The remarks which we ventured to make in the last paragraph of our Results on Solar Physics, recently published, have induced us to examine the Kew Pictures, as regards the decrease of actinic effect from the centre to the circumference of the Sun, to which decrease we may give the name of *atmospheric effect*, since it is without doubt caused by the presence of a comparatively cold solar atmosphere.

In conformity with our views, this atmospheric effect ought to be greater at the epoch of maximum than at that of minimum spot frequency; and furthermore, if there is any reference to ecliptical longitudes in the behaviour of spots—that is to say, if at any time the spots on the Sun attain their maximum at any ecliptical longitude, there ought (according to these views) to be a greater amount of absorbing atmosphere at the same longitude, since such an atmosphere is supposed conducive to the outbreak of spots.

There is reason to think that spots attain their maximum in the ecliptical longitude opposite to that where *Venus* exists, so that we might expect (according to these views) a diminution

in atmospheric effect in the same longitude as *Venus*, and an increase in their effect in the longitude opposite to *Venus*.

If, therefore, *Venus* be at the longitude of the left limb of the Sun, this limb should exhibit less atmospheric effect than the right limb, and if *Venus* be at the right limb we should have most atmospheric effect at the left limb.

It is only the under-exposed pictures that are available for a research of this nature, since an over-exposure tends to do away with the atmospheric effect.

Without giving any hint of our views, Mr. Beckly was requested to select some of the last Kew pictures taken in 1859, a year of maximum spot frequency, and to compare them with those taken in 1864 and 1865, periods of minimum spot frequency; and he came to the conclusion that there was more atmospheric effect in 1859 than in the years 1864 and 1865.

Furthermore, Mr. Stewart, in conjunction with Miss Beckly, has looked over all the pictures taken at Kew from May 1863 to the present date; and this examination was made in such a way that the results could not derive any bias from the opinion of Mr. Stewart, as one of the joint authors of the *Researches on Solar Physics*, above alluded to; for whenever the two observers disagreed, the picture was referred to a third person.

The results of this investigation are given in the following table, and they are at least in conformity with our views, and not antagonistic, while at the same time the evidence is not sufficiently strong to establish conclusively the truth of an hypothesis.

1863.	Left.	Right.	Equal.	
May	2	2	1	} Venus from 80 to 20 degrees to the left.
June	2	1	2	
July	0	7	2	
August	3	3	1	} Venus in conjunction.
September	3	0	2	
October	1	0	2	
November	1	0	3	} Venus about 5-10° to the right.
December	2	2	1	
1864.				
January	4	0	0	} Venus about 45° to the right.
February	-	-	-	
March	3	1	0	
April	3	3	4	} Venus in opposition.
May	-	-	-	
June	1	1	7	} Venus in opposition.
July	4	3	7	
August	1	0	2	} About 150° to the left.
September	3	4	2	
October	0	4	3	} About 140° to the left.

1865.	Left	Right.	Equal.	
November	0	2	3	About 120° to the left.
December	0	1	2	
1865.				
January	0	2	0	" 90° "
February	0	1	0	
March	4	3	3	
April	0	2	11	Venus in conjunction.
May	1	1	10	
June	2	0	1	
July	2	0	3	
August	6	1	5	
September	5	1	8	Venus 90° to the right.
October	8	2	5	

It thus appears, from the above table, which is the result of a joint and careful investigation of the Kew pictures by Miss Beckly and Mr. Stewart that

- (1). When Venus is considerably to the left, there is most atmospheric effect to the right.
- (2). When she is in conjunction or opposition, there is a tendency to equality.
- (3). When she is considerably to the right, there is most atmospheric effect to the left.

A Comparison of the Kew Results of Observations on Sun-Spots with those of Hofrath Schwabe, in Dessau, for the year 1865. By Messrs. Warren De La Rue, Stewart, and Loewy.

Months.	Kew.			Dessau.			Days Ob- served.	Days without Spots.
	New Groups.	Numbers.	Days Ob- served.	New Groups.	Numbers.	Days Ob- served.		Days without Spots.
January	12	632 to 643	5	0	12	1 to 12	23	0
February	10	644 to 653	7	1(?)	11	13 to 23	16	0
March	10	654 to 663	16	1(?)	8	24 to 31	15	1(?)
April	8	664 to 671	15	0	12	32 to 43	30	0
May	13	672 to 684	19	2	10	44 to 53	31	0
June	5	685 to 689	10	1(?)	6	54 to 59	30	1
July	7	690 to 696	13	1	6	60 to 65	31	1
August	8	697 to 704	15	0	7	66 to 72	31	0
September	7	705 to 711	22	2	7	73 to 79	30	7
October	3	712 to 714	15	3	3	80 to 82	31	11
November	5	715 to 719	10	1	6	83 to 88	23	3
December	5	720 to 724	6	1	5	89 to 93	16	2
Total	93	632 to 724	153	13 3(?)	93	1 to 93	307	26

Remarks.—The numbers in the Kew list form a continuation of the Catalogue of Groups, published in the first series of *Researches on Solar Physics*, by Warren De La Rue, Balfour Stewart, and Benjamin Loewy, pp. 6–8.

Those days on which it is doubtful whether spots were on the Sun or not, have the sign (?) after them.

The President stated that the Kew photographs are now taken by Miss Beckly, the daughter of the mechanical assistant of Kew; and it seems to be a work peculiarly fitting to a lady. During the day she watches for opportunities for photographing the Sun with that patience for which the sex is distinguished, and she never lets an opportunity escape her. It is extraordinary that even on very cloudy days, between gaps of cloud, when it would be imagined that it was almost impossible to get a photograph, yet there is always a record at Kew.

Mr. De La Rue remarked that all these investigations on the solar spots occupy a considerable time, and that the results may be interpreted differently by different persons. All that we have to do is to record faithfully the result of our observations; and it is hoped the Kew photo-heliograph will conduce very much to the advance of solar physics. Some time ago some experiments were made by the speaker, in taking solar spots on a very large scale, the pictures of the Sun's disk being on a scale of 3 feet for the Sun's diameter. There are certain difficulties in taking those pictures by means of a reflector; but Mr. Cooke has recently undertaken the construction of a 13-inch refractor, which it is intended to apply to solar and lunar photography.

On a New Method of Mounting Silvered Glass Specula and Diagonal Mirrors in Reflecting Telescopes. By John Browning, Esq., F.R.A.S.

Owing to their cheapness and portability, telescopes constructed with Foucault's silvered-glass mirrors are receiving a constantly increasing share of attention.

The focal length of these mirrors needs not exceed eight times their diameter, while for each inch in diameter—when of the best figure—they will bear a magnifying power of 100 on close stars in finest states of air.

The principal difficulty hitherto encountered in fitting-up these mirrors, when they are of large size, has been that of mounting them in such a manner as to avoid flexure. Various contrivances have been employed for this purpose; among

others they have been supported on air-cushions, on layers of felt, and on a number of triangles, each triangle supported on a centre. All these plans appear to me to possess the common fault, that a blow, or shock of any kind, or even a change in the position of the telescope, frequently throws the speculum out of adjustment. The method I have devised for overcoming this difficulty I shall now proceed to describe.

First, however, I wish to state that I have the mirrors made of glass much thicker than is generally employed for the purpose. For a speculum $6\frac{1}{2}$ inches in diameter I use glass of a thickness of 1 inch, for a speculum 10 inches in diameter glass $1\frac{1}{2}$ inches in thickness. The extra cost of the glass, as it is only ordinary plate-glass, is merely nominal, and the difficulty of working the mirror not at all increased.

After the disk which is to form the mirror is shaped, but before the parabolic figure has been given to it, I work the back carefully to a very perfect plane, by the same method I employ for working the plane surfaces of prisms. When this has been done the mirror has the parabolic figure given to it.

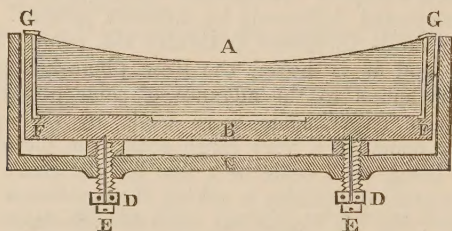


Fig. 1.

A cast-iron cell (B, fig. 1) is now prepared, whose internal diameter is rather larger than the mirror. The bottom of this cell when intended for a 10-inch mirror should be at least three-quarters of an inch thick.

The inside of this cell, on which the mirror will have to rest, I also bring carefully to a plane surface in the following manner. I first turn it with a slide rest in a lathe in the usual way. Then I prepare a Whitworth's plane surface of a circular form, one-eighth of an inch less in diameter than the mirror to be mounted. The bottom of the cell is made flat by scraping, being tested repeatedly during the process with the circular Whitworth's surface.

It greatly facilitates this operation if a groove be turned in the bottom of the cell, at F F, in diagram 1. The centre of the bearing surface of the cell, to the extent of one-third, may be advantageously turned away. This materially lessens the risk of the mirror being deflected, either by the presence of any particles of foreign matter or by the cell altering its form owing to changes of temperature.

For this suggestion I am indebted to the kindness of Colonel Strange. Before the mirror is placed in its cell, the bearing surface is slightly smeared with the best oil, to prevent the oxidation of the iron. Lastly, the mirror is secured in its position by a ring shown at G G in diagram 1.

In adjusting the mirror it is necessary to avoid placing any strain upon the cell, as a very small strain is sufficient to deflect the bottom of the cell, and this deflection extends to the mirror. This is avoided by placing the cell in another cell, and adjusting it in position by means of hollow screws, D D, E E, as shown in the diagram. These screws allow of the cell being raised or lowered without throwing any strain upon the cell. The silvered surface of the mirror is protected by a tightly-fitting cover, which is adapted to the edge of the cell, B, which is turned conical.

Some time since Mr. With, of Hereford, so well known for the success with which he has given his attention to the construction of silvered-glass specula, suggested to me that their performance would probably be improved if some other method were adopted of mounting the diagonal reflector or prism.

As at present mounted on an arm, they are subject to vibration, and the substance of this arm produces coarse rays on bright stars.

I have endeavoured to obviate these inconveniences by the plan of mounting represented in the diagrams figs. 2 and 3.

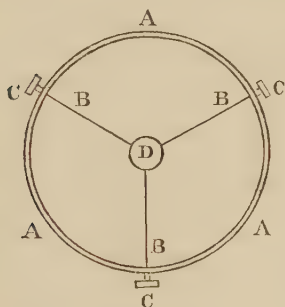


Fig. 2.

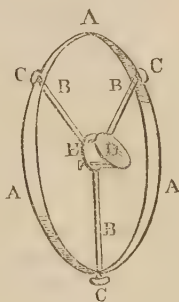


Fig. 3.

In these diagrams fig. 3 is a perspective drawing, and fig. 2 a front view of the arrangement.

The letters are the same for the same parts in both diagrams. D represents the diagonal mirror; B B B, three pieces of chronometer spring stretched tightly, by means of screws through the ring, A A A. This movement is available for centreing. The diagonal mirror or prism, D, is attached by three pillars to a round plate, E, whose diameter is the minor axis of the ellipse. The adjustment is made by means of

hollow screws, of the same kind I have referred to in the commencement of this paper, as applied to the speculum.

The most difficult tests for silvered-glass specula are very bright stars of large magnitude. Their performance on this class of objects is considerably improved by using a good Barlow lens of 4 or 5 inches virtual focus. At the same time, as is well known, the employment of this lens forms a most convenient contrivance for increasing the power of the various eye-pieces. For low powers, achromatic eye-pieces made by combining two achromatic combinations, each consisting of a plano-concave flint and a double-convex crown cemented together, arranged so that the convex side of the crown lenses almost touch each other, answer well. When used in this manner, if the parabolizing operation on the speculum has been conducted with sufficient care and skill, the performance of an $8\frac{1}{2}$ -inch speculum will bear favourable comparison with a 7-inch refractor in point of light, and exceed it in separating power, while the expense need not be more than one-fifth of the cost of such an object-glass.

In conjunction with Mr. Slack, I have contrived a simple and very substantial form of equatorial mounting entirely in cast-iron for these telescopes. At the same time I am greatly indebted to Mr. Slack for the valuable assistance he has rendered me in carefully testing the results of the various modifications I tried in the course of my experiments on mounting specula, before I finally adopted the plans I have just described. With a plain glass mirror as a diagonal reflector, unsilvered, so as to allow the heat-rays to pass freely, these telescopes answer admirably for observations on sun-spots.

Telescopes provided with these mirrors are well adapted for spectrum observations on the stars. From their large aperture, as compared with their focal length, they give abundance of light, and they are not liable to any disadvantages on the score of the want of achromacy.

It is well known there is a considerable number of interesting observations which can only be made with large apertures, and that consideration of expense alone has hitherto prevented their being used.

A damp atmosphere is very prejudicial to the silvered surface of these specula. I have endeavoured to obviate this difficulty, and at the same time prevent the spiral-tube currents by covering the mouth of the telescope with a disk of glass, having plane and parallel surfaces. The production of these parallel plates is attended with considerable difficulty, but I have succeeded in applying one of 8-inches diameter to a mirror, apparently without injuring its performance, but I regret that I cannot speak upon the subject with confidence, as the long continuance of unfavourable weather has prevented me from making any satisfactory experiments.

I shall be highly gratified if the attention I have given to

the mounting of these mirrors and telescopes should lead to their more general adoption.

The President: I think the most important part of the communication is the means of suspending the small mirror. The suppression of a great part of the thickness of the arm tends to do away with the rays which we all know appear to shoot out from the image of a star in the reflector. With respect to the mounting of the large mirror, I think that Mr. Browning's plan would answer very well up to 6 or 8 inches, but the moment one goes beyond that size there must be some such means of suspension as that adopted in Mr. Lassell's telescopes, or there must be flexure; no accuracy of fitting will prevent that.

Observations and Elements of the Comet of December 9, 1865.
By Prof. Donati.

(Translation of a Letter to the President.)

I have determined the following positions of the Comet discovered by the Rev. Padre Secchi at Rome on the 9th December:—

1865. Dec. 11	Florence M. T.			R. A.			Decl.		
	h	m	s	h	m	s	°	'	"
11	7	9	19	22	50	18.44	+4	0	15.9
12	6	44	20	22	52	12.55	3	56	43.8
13	8	11	30	22	54	16.70	3	52	5.3
14	8	16	20	22	56	14.72	3	47	36.5
15	8	16	46	22	58	13.90	3	43	12.2
16	8	23	49	23	0	14.74	3	38	26.9
17	8	52	34	23	2	17.89	3	33	16.0
18	8	28	40	23	4	17.19	3	28	13.2
19	8	9	28	23	6	15.84	3	22	53.1
20	8	14	47	23	10	28.20	3	17	11.1
21	8	18	58	23	10	28.20	+3	11	12.5

The Comet is of extraordinary faintness. Using the observation at Rome of the 9th December, and my own observations of January 13 and 19, I have calculated the following elements:—

$T = 1866, \text{ Jan. } 20^{\circ} 32' 06'' \text{ Greenwich M.T.}$

$$\left. \begin{array}{l} \Omega = 205^{\circ} 15' 39'' \\ \pi = 31^{\circ} 22' 31'' \\ i = 12^{\circ} 14' 10'' \end{array} \right\} \text{Mean Equinox, 13 Dec. 1865.}$$

$$\log. q = 0.28886$$

This orbit satisfies exactly the mean observation, but it gives for the observation of Dec. 21 the following errors:—

	Obs. — Cal.
R. A.	Decl.
23'.7	+ 8'.9

Although the elements are only a rough approximation, we see that the orbit is quite different from that of the Comet of Biela, especially in the perihelion distance and in the position of the orbit in its plane. But the new Comet moves in a plane not very different from that of the Comet of Biela, and passes its perihelion nearly at the same time. These coincidences and the circumstance of not finding the Comet of Biela at its place may be the result of accident, or may have a physical reason. It seems to me that the mysterious phenomena which have been already remarked in Biela's Comet permit the asking of this question—which at present I cannot answer; but it may be hoped that science will give a solution of it.

Florence, 22 Dec. 1865.

Occultation of 115 Tauri by the Moon.

By C. G. Talmage, Esq.

I beg to enclose the times of the occultation of 115 *Tauri* on 1865, December 30:—

	h	m	s
G.M.T. of Disappearance = Dec. 30	7	34	55.60
„ Reappearance	8	21	54.35

Both times are exact, observed with a power of 85 on the 10-inch Equatoreal.

Mr. Barclay's Observatory, Leyton,
1866, January 11.

Observations of η Argûs. By J. Tebbutt, Jun. Esq.

In accordance with the promise contained in my letter of the 17th July last, I now send you my observations of the remarkable southern star η Argûs. The comparisons have all been made with the naked eye, unless where otherwise stated, and under the most favourable conditions of sky, &c. The first comparisons were made in July 1854; but it appears from my journal that η Argûs was employed by me as a comparison-star on the 30th September and 12th October, 1853, in sextant observations of a large comet at that time visible in the morning sky. It was then a reddish star of the first magnitude.

1854, July 5. Of α and β Centauri, α Crucis, and η Argûs, the first was by far the brightest; β Centauri and η Argûs were very nearly equal, but I think the latter was somewhat the brighter. η Argûs somewhat exceeded α Crucis.

1860, May 4. 8 P.M. η Argûs was less than β Canis Majoris and about equal to β Canis Minoris. The standard stars were, however, too near the horizon for a good comparison, and the Moon was very bright. It was also about equal to δ Crucis and θ Argûs.

1860, May 18. 6^h 30^m P.M. About equal to β Canis Minoris, but not quite so bright as β Canis Majoris.

1862, January 26. Not so bright as β Canis Minoris or θ Argûs; perhaps slightly less than σ Orionis, and exactly equal to B.A.C. 3740.

1862, June 23. About equal to ϵ Crucis and B.A.C. 3526 and 3619, and considerably less than δ Crucis or θ Argûs.

1862, July 15. Greater than B.A.C. 3688 and less than B.A.C. 3740. The same result was obtained with a small telescope.

1862, July 22. η Argûs was of a magnitude intermediate between B.A.C. 3740 and 3688, and very little brighter than ζ and η Crucis.

1862, July 24. 9^h 23^m. Slightly less than B.A.C. 3740 and 1818, and exactly equal to B.A.C. 3594.

1862, July 25. 6^h P.M. Less than B.A.C. 3740 and 3818, equal to B.A.C. 3594, and greater than B.A.C. 3655 and 3688.

1862, August 28. 7^h 20^m P.M. Greater than B.A.C. 3688; equal to B.A.C. 3594; and less than B.A.C. 3740.

1863, April 15. 10^h P.M. Greater than B.A.C. 3655 and 3688, and less than ϵ Crucis and B.A.C. 3740 and 3818. It was decidedly a shade less brilliant than B.A.C. 3594.

1863, April 16. 11 P.M. Somewhat less than ω^1 and ω^2 Scorpii and B.A.C. 3594.

1863, May 24. 8^h 15^m P.M. Less than ϵ Crucis and B.A.C. 3740 and 3818; greater than B.A.C. 3688; and about equal to

ω^1 and ω^2 *Scorpii*. It was decidedly a shade less brilliant than B.A.C. 3594, which was very nearly equal to B.A.C. 3818.

1864, March 23. 9^h P.M. Equal to B.A.C. 3655 and 3688, both by the naked eye and the telescope. Full Moon.

1864, April 23. 6^h 30^m P.M. A shade less than B.A.C. 3655 and 3688; it was certainly not greater. It was brighter than B.A.C. 3642, which could only be seen by oblique vision.

1864, May 31. 7^h P.M. About equal to B.A.C. 5501, a small star a little north of *Antares*, and about equal to or perhaps slightly inferior to B.A.C. 3655 and 3688; it was superior to B.A.C. 3642.

1864, July 28. 7^h P.M. Somewhat inferior to B.A.C. 3655 and 3688, and slightly superior to B.A.C. 3642.

1865, February 22, 23, and 24. 9^h P.M. η *Argûs* was certainly inferior to B.A.C. 3655 and 3688; the stars were at a great altitude.

1865, March 14. 9^h 20^m P.M. η *Argûs* was distinctly visible to the naked eye, notwithstanding the Moon. It was certainly not quite so bright as B.A.C. 3655 or 3688.

Windsor, New South Wales,
October 19, 1865.

Elements of Comet I. 1865. By J. Tebbutt, jun. Esq.

I also avail myself of this opportunity to forward my corrected elements of the Great Comet of this year; they agree pretty well with the series of observations made at Melbourne and Windsor.

$$T = 1865, \text{ January } 14^d 32520 \text{ G.M.T.}$$

$$\left. \begin{array}{rcl} \pi & 4^\circ 50' 24'' & \\ 88 & 253 & 3 \text{ } 10' 4'' \\ i & 92 & 28 \text{ } 20' 0'' \end{array} \right\} \text{M. Equinox, } 1865^o.$$

$$\log q = 8.4147561$$

Windsor, New South Wales,
October 19, 1865.

There is contained in the *Comptes Rendus* of the French Academy, .t. lxi. (December 1865), an interesting paper by M. Delaunay—"Sur l'Existence d'une Cause Nouvelle ayant une influence sensible sur la valeur de l'équation séculaire de la Lune." The substance of this paper is as follows:—

Assuming that the secular acceleration of the mean motion of the Moon, as indicated by observation, is notably greater than that occasioned by the variation of the excentricity of the Earth's orbit, it becomes necessary to seek for a new cause for the excess in question, or portion not accounted for by Laplace's theory. A progressive cooling of the terrestrial globe would produce a diminution, not an increase, of the acceleration, and the effect is therefore not to be so accounted for. But M. Delaunay considers that he has found a cause to which it is natural to attribute the excess in question, a cause, that is, which produces a progressive retardation in the rotatory motion of the Earth, and consequently an apparent acceleration in the mean motion of the Moon.

We know that the Moon by its action on the waters of the ocean occasions therein an oscillatory motion constituting the phenomenon of the tides. The Sun concurs in the production of this phenomenon, but, to avoid complication, the solar action may be disregarded. The form of the surface of the sea changing continually, it follows that the action of the Moon on the entire mass of the Earth (including therein the waters of the sea) is at every instant somewhat different from what it would be if the phenomenon of the tides did not exist; in seeking to explain wherein consists the difference, it is seen that *it consists principally in a couple acting constantly on the Earth in a direction contrary to the rotatory motion*, and that the result is a progressive diminution of the angular velocity of the terrestrial globe.

To further explain this, imagine, in the first instance, that the Earth is entirely covered by the waters of the ocean. In virtue of the action of the Moon, the waters tend to elevate themselves above their mean level in the two opposite regions situate at the extremities of the diameter directed towards the centre of the Moon. Admitting for greater simplicity that, without this action of the Moon, the surface of the sea would be exactly spherical, and that the Moon is in the plane of the Equator, then, in virtue of the lunar action, the surface of the sea tends constantly to assume the form of an ellipsoid of revolution, having the same centre as the sphere, and its transverse axis directed in the line joining the centres of the Earth and Moon. The effect of the Earth's motion of rotation is that this ellipsoid moves in regard to the Earth exactly as the Moon in its diurnal revolution, since the axis of the ellipsoid is always directed towards the Moon. But this continual dis-

placement causes that the surface of the waters is never coincident with that of the ellipsoid of equilibrium—the frictions and resistances which the waters suffer in their motion produce the effect that the axis of the elongated figure of the waters at any instant *lags* behind that of the ellipsoid of equilibrium wherewith it tends to coincide. Without this retardation, the high tide would take place at the instant of the passage, superior or inferior, of the Moon over the meridian; in consequence of the retardation, the high tide takes place, not at the instant of the passage, but some hours after it.

The phenomenon is more complicated in the actual case, where the terrestrial surface is not completely covered by the waters, but whatever the distribution of land and water may be, the frictions and resistances of every kind which the waters suffer in their motion produce an effect similar to that indicated in the first-mentioned case. There is always a retardation in the oscillatory movement. Speaking generally, the oscillatory movement is what it would be if these resistances did not exist, and if the Moon was at every instant in a position in the heaven *behind* the actual position, in regard to the direction of the apparent diurnal motion.

Returning to the more simple case where the surface is completely covered, let us consider how the action of the Moon is modified in consequence of the elongated form produced in the waters of the sea by this same action. There exist, as it were, two fluid prominences, situate at the extremities of a diameter directed, not towards the Moon itself, but towards a point in the heavens situate at a certain distance eastwardly from the Moon. These two prominences are at unequal distances from the Moon, the one nearer, the other further than the centre of the Earth. Bearing in mind the explanation of the lunar action which produces the tides, it is easy to see that the nearer prominence is, as it were, attracted to, the further prominence repelled by, the Moon. The resultant action is that of a couple applied to the mass of the Earth, and tending to make it rotate in the direction contrary to that of its actual motion—a couple which therefore tends to retard the motion of rotation.

M. Delaunay remarks in a note that this idea of the resistance which the Moon continually opposes to the rotatory motion of the Earth, in consequence of its action on the waters of the sea, has been referred to in several printed works; but that it has been always assumed that the effect is too small to be sensible. He recalls that the object of his communication to the Academy was not to make known this cause of retardation, but to show—1. That the resulting retardation is far from insensible; 2. That it is possible to see therein a complete explanation of the portion, not accounted for by Laplace's theory, of the lunar acceleration.

Returning to the discussion, and imagining to fix the ideas

that the retardation of the time of high water is three hours, or, what is the same thing, that the diameter through the two prominences is inclined at an angle of 45° to the line directed to the centre of the Moon, let us calculate the effect which the couple produces, replacing the two liquid prominences by material points situate at the extremities of the oblique diameter in question.

Denoting by T the centre of the Earth; by L that of the Moon; by E the extremity of the equatoreal radius inclined at an angle of 45° to the line TL eastwardly; and by E' the opposite extremity of this diameter; let M be the total mass, m the mass of the Moon, and μ that of each of the material points supposed to be placed at E and E' respectively. Suppose moreover that R is the distance TL, taken to be constant, of the centres of the Earth and Moon, r the terrestrial radius TE or TE', and d the distance of the centre of the Moon from the point E. Taking f as the unit of attraction, we have

$$\frac{fm\mu}{d^2}$$

for the attraction of the Moon on the material point of mass μ at E. To refer the motion of the terrestrial globe to axes fixed in direction through its centre of gravity, we must join to this a force $\frac{fm\mu}{R^3}$ acting along the line LT in the direction from L to T. We have, moreover,

$$d^2 = R^2 + r^2 - 2Rr \cos 45^\circ = R^2 + r^2 - \sqrt{2}Rr.$$

The resultant of these two forces constitutes the relative action of the Moon on the mass μ at E: taking the sum of these moments in regard to the centre of the Earth, and neglecting smaller terms, we find $\frac{3}{2} \frac{fm\mu r^2}{R^3}$; for the expression of the moment in question.

The same expression gives also the moment in regard to the second mass μ at E'. The total moment produced by the attractions on the two masses μ at E and E', tending to retard the motion of rotation, is therefore equal to $\frac{3fm\mu r^2}{R^3}$, and consequently the differential equation for the motion of rotation, ω being the angular velocity and I the moment of inertia of the Earth in regard to a diameter, is

$$\frac{d\omega}{dt} = -\frac{3fm\mu r^2}{IR^3},$$

and taking the Earth to be homogeneous, we have

$$I = \frac{2}{5} M r^2.$$

Moreover, by considering the motion of the Moon about the Earth, and disregarding μ in comparison of M , we find $f = \frac{4\pi^2 R^3}{T^2 M}$, where T is the period of the sidereal revolution of the Moon about the Earth. Introducing the values of I and f , the differential equation becomes

$$\frac{d\omega}{dt} = -3 \frac{\pi^2 m}{T^2} \frac{\mu}{M} \cdot \frac{\mu}{M},$$

and, integrating twice, it appears that the total angle of rotation during the time t is, in consequence of this action of the Moon, less than it would otherwise be by a quantity A , the expression of which is

$$A = 15 \pi^2 \frac{m}{M} \cdot \frac{\mu}{M} \frac{t^2}{T^2}.$$

Let us now inquire what is the value to be attributed to these masses μ , in order that the retardation A corresponding to a time t equal to a century, may give rise to an apparent acceleration equal to 6 seconds (which is about the value of the portion not accounted for by Laplace's theory). We must for this purpose suppose A equal to the angle described by the Earth in its rotatory motion, while the Moon advances 6 seconds in its mean motion round the Earth; A is therefore equal to 6 seconds multiplied by $27\frac{1}{3}$ or to 164 seconds.

Effecting the calculation, and taking $\frac{1}{80}$ for the ratio of the Moon's mass to that of the Earth, we find

$$\mu = \frac{1}{4160\ 000\ 000} M.$$

To gain a more distinct notion of the magnitude of this mass, assume that it is the mass of a volume V of water; then, if 5.5 is the mean density of the Earth, we find

$$V = 1429\ 000\ 000\ 000 \text{ cubic metres,}$$

or, if this mass of water of the volume V has the form of a stratum on a circular base of the uniform thickness of one metre, the radius of the base will be about 675 kilometers; that is to say, such a stratum applied to the surface of the Earth would occupy a breadth of about 12 degrees at the equator: the magnitude of this stratum of water is obviously

comparable with those of the fluid prominences which the action of the Moon would produce in the hypothetical case in question.

In presence of the foregoing result, obtained, indeed, on an hypothesis very different from the actual circumstances, it is, as M. Delaunay considers, impossible not to admit that an analogous effect of a sensible magnitude is produced by the Moon on the waters of the ocean.

The Sun, which contributes to produce the phenomenon of the tides, though in a less degree than the Moon, should equally contribute to this progressive diminution of the rotatory motion of the Earth.

The author states as follows his general conclusion :—

The perturbing forces to which are due the periodic oscillations of the surface of the sea (phenomenon of the tides) in exercising their action on the fluid intumescences which they occasion, cause a progressive retardation of the movement of rotation of the Earth, and thus produce a sensible apparent acceleration in the mean motion of the Moon.

He remarks that the foregoing result is not in accordance with what Laplace has found in investigating the influence that the state of fluidity of the waters of the sea may have on the movement of the terrestrial globe considered as a whole. Laplace says formally that this state of fluidity of the sea does not affect the uniformity of the rotation of the globe (*Mécanique Céleste*, book v.) But it is to be remarked that to arrive at this conclusion, Laplace confines himself to quantities of the first order in regard to the perturbing forces considered by him. It was, therefore, impossible for him to find the retardation of the movement of rotation, the real existence whereof has just been established, since this retardation is evidently of the order of the square of the perturbing forces in question.

The exact calculation of the retardation due to the combined action of the Moon and Sun, would require a knowledge of all the circumstances of the tides as well along the shores as in mid-ocean. Such a direct calculation is impossible; the actual retardation can only be found indirectly by means of the lunar acceleration, as determined by observation; and this gives a new interest to the comparisons of the lunar tables with the ancient eclipses, in the view of thereby arriving at the true value of the lunar acceleration.

The President called attention to two very important works among the presents; one from Professor Hansen and the other from M. Delaunay.* The volume contributed by Pro-

* The above-mentioned memoir in the *Comptes Rendus*.—Ed.

fessor Hansen relates to the Lunar Theory, and is the second volume of the important work undertaken by that distinguished physical astronomer. This volume will afford an opportunity of studying the theory of Hansen with regard to certain inequalities of the lunar motion, and of comparing his investigation of the lunar acceleration with that of Mr. Adams. The coefficient of the lunar acceleration obtained by Prof. Hansen is much higher—about double that obtained by Mr. Adams; and a short time back there was a great controversy on the subject of the correctness of the one or the other. However, it seems to be established that Mr. Adams' coefficient is correct; but there can be no question that it does not fit exactly with the records of the ancient eclipses. M. Delaunay now steps in opportunely and proposes to account for this difference by the assumption that there is a retardation of the period of rotation of the Earth produced by the action of the Moon on the waters of the Earth, and causing the Earth to rotate more slowly by about 100th of a second than it did 2000 years ago. Whether the hypothesis will or not be at once accepted as true, it is certain that everything which comes from M. Delaunay must be regarded with the highest respect.

Mr. Stone said as follows:—The question discussed by M. Delaunay in this paper is a most interesting and important one. To clear up my own ideas on the subject, I have tried the question in a slightly modified, but, I believe, in a strictly analogous form. Suppose the Earth a sphere; the Moon to move in the plane of the equator; the Earth surrounded by a small *solid* shell of the mean density of the sea; the outer surface of this shell to assume the form of a prolate spheroid. Then, if the shell moves in such a manner that its greatest axis always lags three hours behind the Moon, the same assumption as that made by M. Delaunay, we have a rough geometrical representation of the "lunar tides." The shell can only move in this constrained manner through the action of friction between the shell and the Earth. I have first determined the moment of friction required to produce the constrained motion of the shell, and then the dimensions of the shell required to produce a diminution in the Earth's velocity of rotation, such that from it there would result an apparent acceleration of $6''$ in the Moon's mean motion. I find that a high tide of only 2 inches would be sufficient to produce the required retardation of the Earth's velocity of rotation. My result closely agrees with M. Delaunay's; my mass is even larger than his. This is as it should be, from the distribution of the mass in my problem. I would, however, remark that the case here considered is essentially that of a *solid shell*. Every particle of this shell has to move around the Earth in each month. I fear that the motion of a fluid mass under like conditions would be so exceedingly different that it would be

very difficult, if not impossible, to infer anything like quantitative results respecting the fluid case from the solid case of our problem. I cannot think that any mere comparison of the matter in the protuberant parts of the solid spheroid and fluid spheroid gives us sufficient information on this point. No doubt, when pushed to the second order of small quantities, and therefore in an accurate solution, some retarding effect would be produced in the case of a fluid mass moving with high water lagging behind the Moon. The mean positions of the oscillating particles of fluid would be bodily transferred. We should have for the relative velocities of the fluid mass perpendicular to the Earth's axes non-periodic terms, and from such terms there would arise a permanent frictional moment on the Earth tending to destroy its velocity of rotation; but the question is, to what extent would this be the case. I cannot see that M. Delaunay's paper gives us any information on this point. I cannot therefore accept it as a demonstration of a retardation of the Earth's rotation sufficient to produce sensible effects on the apparent secular acceleration of the Moon. Whether the action of the friction between the Earth and the sea, necessary to produce the observed retardation of the tides, does produce a sensible effect on the length of the day or not, will, I fear, have, for the present, to be decided by the agreement or non-agreement of the theoretical value of the secular acceleration of the Moon's mean motion calculated on the assumption of the invariability of the sidereal day, with the observed value. Whether we accept or not the results of M. Delaunay's paper, there can be no difference of opinion respecting its value in recalling attention to the effects of the phenomena of the tides on the Earth's rotation.

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